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RADIOTELEGRAPHY.—*Long distance radio receiving measurements in 1924.* L. W. AUSTIN. Laboratory for Special Radio Transmission Research.[2]

Two stations, Monte Grande (LPZ), Argentina, and Cayey (NAU), Porto Rico, have been added during the year to the number of those regularly measured in Washington. Monte Grande is interesting; first, because it is 2,000 km. farther away than the European stations, and second, because the waves travel in a south-north direction from the southern to the northern hemisphere; thus producing entirely different seasonal conditions from those encountered in the transmission from Europe to America. The station gives nearly the same morning intensity as Nauen, Germany, and the ratio of average observed to calculated values is about three to one. Unfortunately Monte Grande does not send in the afternoon.

Cayey has been observed partly because its frequency, approximately 33.8 kc. (8,870 m.), is considerably higher than the other stations and partly on account of its nearly south-north direction of transmission which at certain seasons lies nearly parallel to the sunset shadow wall. It was thought that this might cause eccentricities in reception at about sunset, but no peculiarities have been observed on the rather limited number of occasions when transmission took place at that time.

The mean monthly values of the field intensities of the signals from the various stations, and of the corresponding atmospheric disturbances, are shown in the tables and curves.

Table 1 gives the approximate data concerning the transmitting stations, as far as known.

[1] Published by permission of the Director of the Bureau of Standards of the U. S. Department of Commerce.

[2] Conducted jointly by the Bureau of Standards and the American Section of the International Union for Scientific Radio Telegraphy.

Tables 2 and 3 give the monthly averages of the received field intensities and of the corresponding atmospheric disturbances in microvolts per meter. It is to be remembered that the signals received in Washington at 10 A.M. from Europe have an all daylight path

TABLE 1.—APPROXIMATE TRANSMISSION DATA

	FREQUENCY f	WAVE LENGTH λ	ANTENNA CURRENT I	EFFECTIVE HEIGHT h	DISTANCE d
	kc.	*m.*	*amp.*	*m.*	*km.*
Nauen POZ[1]	23.4	12,800	390	145	6,650
Bolinas KET	22.9	13,100	420	51	3,920
El Cayey NAU	33.8	8,870	150	120	2,490
Monte Grande LPZ	23.6	12,700	610	150	8,300
Lafayette LY	15.9	18,900	475	180	6,160
Ste. Assise UFT	20.8	14,400	380	180	6,200
Ste. Assise UFU	15.0	20,000	475	180	6,200
Malabar PKX	19.0	15,800	500	320	14,700
Cavite NPO	19.3	15,500	180	120	11,800

[1] During the year Nauen has used at times an antenna with h. = 175 m. and a current varying between 300 and 480 amperes for its 23.4 kc. frequency.

TABLE 2.—AVERAGE SIGNAL AND ATMOSPHERIC DISTURBANCE INTENSITIES IN 1924 FOR LAFAYETTE (LY), STE. ASSISE (UFU), AND EL CAYEY (NAU) IN MICROVOLTS PER METER

1924	A.M.			P.M.			A.M.		P.M.	
	LY	UFU	Dist.	LY	UFU	Dist.	NAU	Dist.	NAU	Dist.
January	130.0	63.5	21.2	160.0	89.6	26.5	—	—	—	—
February	153.0	64.2	39.3	125.7	71.5	70.6	—	—	—	—
March	117.5	50.3	30.2	88.3	46.7	70.4	—	—	—	—
April	136.7	50.9	65.8	88.2	34.7	166.5	73.2	17.7	59.3	47.3
May	107.5	52.2	97.3	75.8	34.7	180.0	79.8	23.3	59.9	64.4
June	120.0	45.8	105.4	77.3	36.6	605.0	57.3	35.4	43.5	170.0
July	113.6	47.1	56.0	61.8	22.5	267.0	112.5	30.0	66.5	137.0
August	93.5	40.3	87.0	52.5	17.7	294.0	57.0	42.0	73.2	157.0
September	119.7	55.3	50.0	88.6	35.3	151.0	100.2	19.0	92.5	88.0
October	113.7	54.4	46.0	137.4	57.0	110.0	87.0	10.0	67.6	31.0
November	87.8	37.4	38.3	180.9	66.3	66.0	62.8	10.8	65.1	14.0
December	87.6	50.3	30.2	151.5	64.2	35.9	56.1	7.1	62.1	7.8
Average	115.0	50.9	55.5	107.3	48.0	170.2	76.2	21.7	65.5	79.6

although during the short days of winter they are probably disturbed by being transmitted too close to the European sunset time. The 3 P.M. signals are sent during the evening hours and during the winter considerable parts of their paths lie in darkness.

Fig. 1 shows the monthly averages of the 10 A.M. signals from Bor-

TABLE 3.—AVERAGE SIGNAL AND ATMOSPHERIC DISTURBANCE INTENSITIES IN 1924 FOR STE. ASSISE (UFT), BOLINAS (KET), NAUEN (POZ), AND MONTE GRANDE (LPZ) IN MICROVOLTS PER METER

1924	A.M.					P.M.				
	UFT	KET	POZ	LPZ	Dist.	UFT	KET	POZ	LPZ	Dist.
January	37.3	—	16.5	—	17.7	39.3	—	24.0	—	22.1
February	41.8	80.4	19.3	—	32.8	37.4	69.6	21.8	—	61.3
March	40.7	59.9	35.7	40.1	24.0	32.5	54.4	37.0	—	58.9
April	39.6	56.3	28.5	33.6	53.3	21.2	47.4	17.8	—	136.0
May	34.3	57.4	22.5	27.2	77.3	21.4	40.6	13.1		158.0
June	37.0	55.4	27.1	26.1	90.4	21.5	35.0	18.9		531.0
July	43.1	53.5	30.3	33.3	50.0	22.6	26.1	15.1		238.0
August	40.4	24.5	34.9	41.3	78.0	21.8	36.3	15.7		306.0
September	59.5	58.9	49.6	39.3	54.0	36.5	50.8	31.2		148.0
October	49.9	62.7	31.2	42.3	31.0	44.0	59.8	32.0		81.0
November	24.3	49.3	14.5	38.4	26.0	39.0	61.2	43.8		56.0
December	32.7	54.2	21.7	46.7	23.5	41.4	54.1	39.4		25.9
Average	40.0	55.6	27.6	36.8	46.5	31.5	48.6	25.8		156.0

deaux for the years 1922, 1923, and 1924. Fig. 2 gives similar 10 A.M. curves for Nauen. Fig. 3 shows the morning signals for El Cayey and

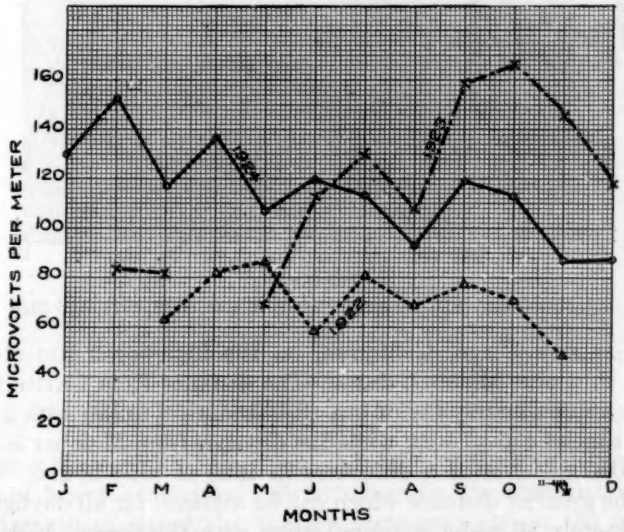


Fig. 1.—Lafayette (LY) average signal, 10 A.M., 1922, 1923, and 1924

Monte Grande. Fig. 4 shows the variations in the monthly disturbance averages at 3 P.M. for three frequencies. The marked difference

between the disturbances at 24.0 kc. and 33.3 kc. is noticeable. In Fig. 5 the 3 P.M. disturbances for a frequency of 24.0 kc. (12,500 m.) are plotted for the years 1922, 1923, and 1924. Fig 6 gives similar curves for a frequency of 15.0 kc. (20,000 m.) for the same years.

These curves for 1922, 1923 and 1924 are given merely as information. It is too early to attempt to draw any definite conclusions from their variations.

Field intensity measurements were made during August and Sep-

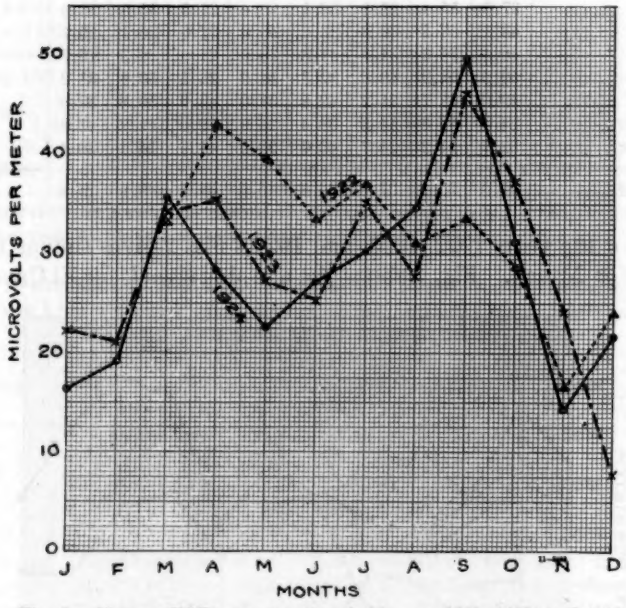


Fig. 2.—Nauen (POZ) average signal, 10 A.M., 1922, 1923, and 1924

tember at San Diego, California, on the high-power arc stations, Cavite, P.I., and Malabar, Java. The distance from Cavite to San Diego is approximately 11,800 km. (6,400 nautical miles) with a difference in time of eight hours, while the distance from Malabar is 14,700 km. (8,000 miles) with a difference in time of nine hours. This is about the greatest distance which can be attained for all daylight and approximately all water communication with the present high-power stations of the world. Even in this case there are only about two hours during the day available for observations without too close approach to sunset or sunrise at one station or the other. The observations

were taken with the telephone comparator and the apparatus calibrated with a radio-frequency generator and attenuation box as in the Western Electric method of measuring signals.

The final results were as follows:

	CAVITE	MALABAR
Observed averages	2.04 μv/m	4.02 μv/m
Calculated (Austin-Cohen formula)	0.69	1.83

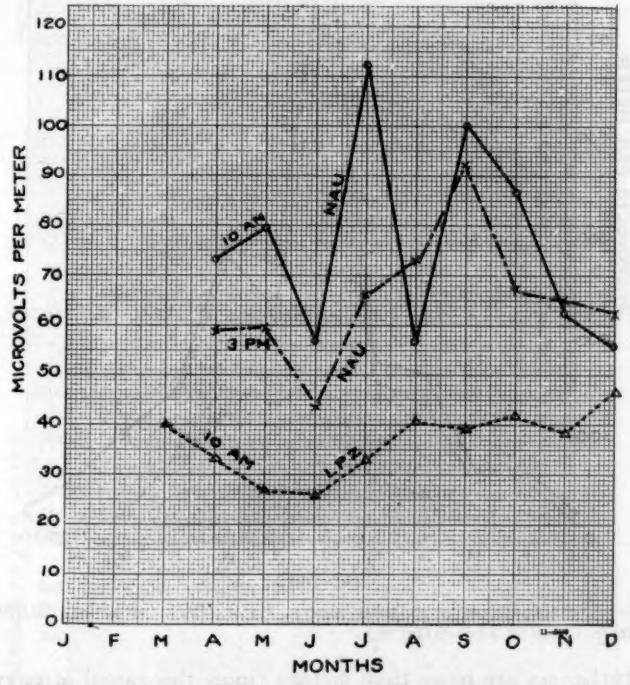


Fig. 3.—El Cayey (NAU) and Monte Grande (LPZ) average signal, 10 A.M., 1924

During the year experiments have been carried on to determine the effect of heavy atmospheric disturbances on the observed values of the strength of signals by making measurements on the telephone comparator, first with an artificial antenna and then with an elevated antenna on which the disturbances were coming in. It was found: (1) That if the disturbances were separated by intervals of compara-

tive silence, the readings were independent of the intensity of the disturbances provided the telephones were removed from the ears sufficiently to prevent the deafening effect of the crashes. (2) If the disturbances were practically continuous but less than about seven times the strength of the signal, the observations were unaffected. (3) With continuous disturbances between seven and sixteen times the strength of the signal the observed values are too low. (4) When

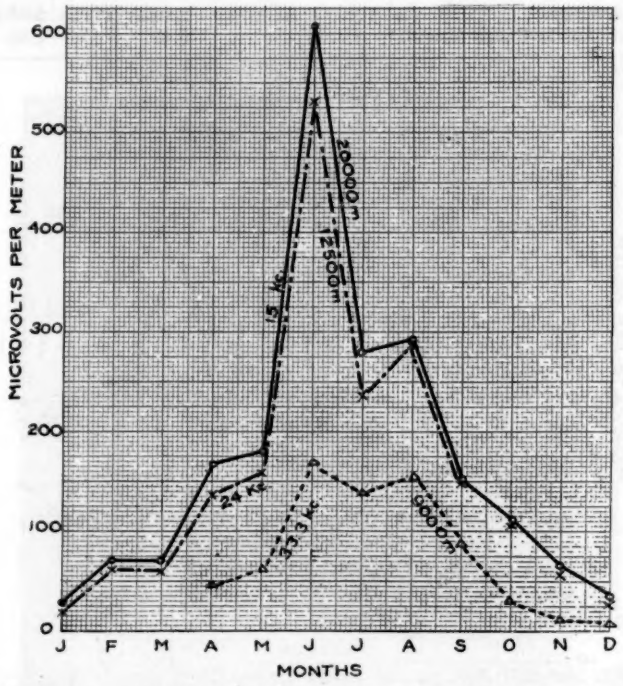


Fig. 4.—Average atmospheric disturbances, 3 P.M., 1924, for 15 kc. (20,000 m.), 24 kc. (12,500 m.), and 33.3 kc. (9,000 m.).

the disturbances are more than sixteen times the signal strength, the signal is not heard. These experiments have made it possible to make estimates of the signal strength of the weaker stations on the summer afternoons instead of arbitrarily throwing them out, or considering them inaudible. This is a matter of some importance for the determination of the summer afternoon fading. The application of these corrections to the afternoon observations of 1922 and 1923 practically doubles the average values of the summer afternoon readings of the weaker stations, like Nauen.

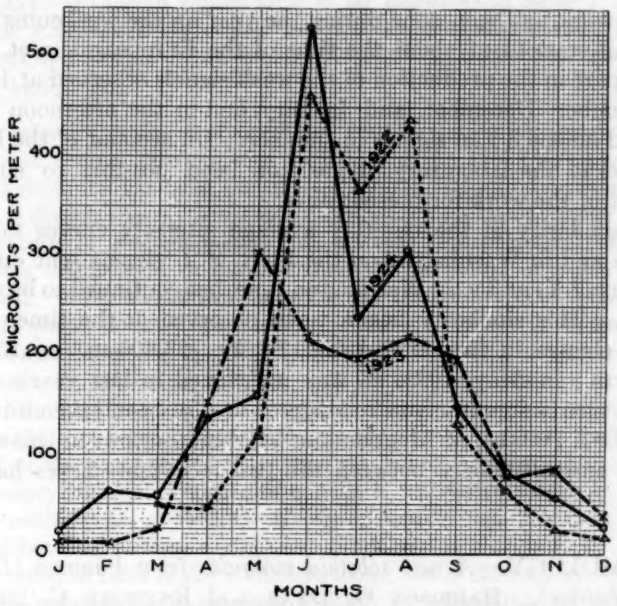


Fig. 5.—Average atmospheric disturbances, 3 P.M. for 1922, 1923, and 1924. f = 24 kc. (12,500 m.).

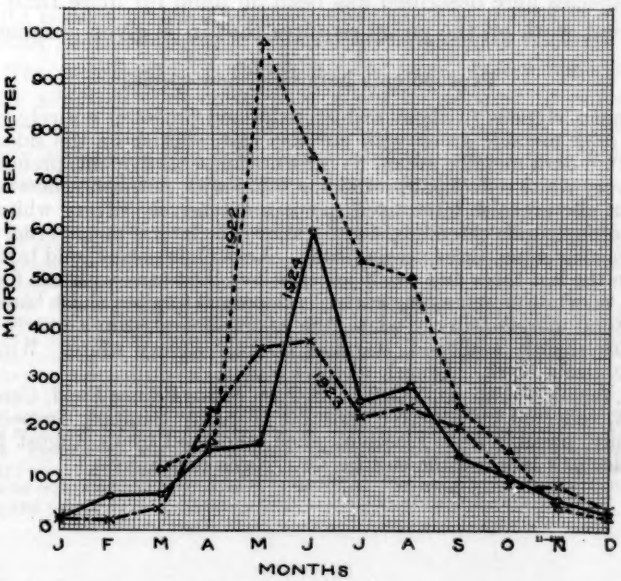


Fig. 6.—Average atmospheric disturbances, 3 P.M. for 1922, 1923, and 1924. f = 15 kc. (20,000 m.).

Some work has been done during the year on the weakening of the European stations at about the time of the European sunset. This plays a part in the production of the weak signals observed at 10 A.M., in November, December, and January, and in the afternoon fading observed on the 3 P.M. signals in summer. On account of the limited personnel of the laboratory, it has not been possible to complete this part of the work for presentation.

The similarity in the monthly average intensity curves taken at Meudon and in Washington on the U. R. S. I. signals sent out from Bordeaux (LY) at 3 P.M., Washington time, has continued to be worthy of note. This similarity began to be observed at the time of Bordeaux's change in frequency from 12.8 kc. (23,400 m.) to 15.9 kc., (18,900 m.) in May, 1923, as was mentioned in last year's report. Similar, nearly simultaneous readings have also been taken on Rocky Point, L. I. (WQL), at Meudon and Washington, but in this case no definite correspondence between the two reception curves has been found.

ENTOMOLOGY.—*A new sabethid mosquito from Panama (Diptera, Culicidae)*. HARRISON G. DYAR and RAYMOND C. SHANNON (Communicated by S. A. ROHWER).

The species here described has been on hand for more than a year, and recent work on the group requires that it be given a name.

Prosopolepis hemisiris, new species

Antennae, clypeus (without scales), palpi and proboscis entirely blackish; occiput dark metallic green with purplish cast, whitish on the sides below; mesonotum dark metallic green, no prescutellar setae evident, pronotal areas with silvery white reflection; prothoracic lobes with purplish reflection dominant, but also greenish intermixed; pleurae extensively silvery white scaled; lower sternopleurals extending slightly above the lateral metasternal sclerite; two spiracular setae, three prealar setae. Legs bluish black; mid tarsi silvery white on the last three and a half joints below; hind tarsi with last four joints missing, no white on remaining parts. Abdominal tergites bluish black above, their lateral margins silvery white, the colors separated in a nearly straight line, very slightly scalloped, sternites entirely silvery white. Wing scales broad, entirely black. Length about 4.5 mm.

Type, female.—Cat. no. 28210, U. S. Nat. Mus.; France Field, Canal Zone, Panama, collected by a native assistant of Army Sanitary Inspector J.B. Shropshire, possibly from a coconut shell, and bred by us August 16, 1923. The collector's label on the culture was "cocoa mosquitoes."

BIOLOGY.—*Biological relationships of the mathematical series 1, 2, 4, etc., with a description of a new nema,* Tylenchus cancellatus, (Contributions to a Science of Nematology XV). N. A. COBB, U. S. Department of Agriculture.

The behavior of the components of matter, e.g., in chemical reactions, appears to compel discontinuous variation in the evolution of organisms. Organic evolution has been thought continuous, but mutation now suggests that it is discontinuous. Must it not necessarily be discontinuous[1] from the very nature of the composition of matter? Morphological changes in organisms originate in chemical changes in the *matter* of which they are composed. Now, a chemical change is one that either takes place or does not take place; nothing intermediate is known. Hence it seems that the fundamental changes in the evolution of organisms, so far as we can conceive at present, i.e., chemical changes, *must be saltatory.* But we cannot conceive of the greater and obvious (visible) changes, except as summations of these minute changes. The visible changes then must *per force* be considered of the same character as that of their components, i.e., all visible evolutionary changes in organisms *must be* of a saltatory nature.

The mathematics of the morphology of organic evolution may therefore be considered as, at least mainly, discontinuous,—arithmetical.

The material basis of life is discontinuous, but is the only known form of matter so organized as to grow and multiply by assimilation; in this lies the fundamental difference between living objects and all others;—not a mathematical difference.

Matter is dual, or less abstractly, there exists in matter an exceedingly widespread, probably universal, "bipolarity", exemplified, therefore, in organisms. The universality of "bipolarity" is more or less understood and generally admitted. Its universality might be assumed to prove, and at least very strongly suggests, its necessity. Assuming its necessity, this bipolarity determines that *cells, as well as many of their components,* multiplying, do so by binary division in a bipolar manner.[2]

[1] *Mathematics.* Arithmetic and its derivatives arose through everyday problems connected with matter, which is *discontinuous.* The Calculus, mathematics of continuity, arose through problems like those of astronomy, where the *continuity* of space and time impress us most vividly.

Quantity. It may be said we cannot conceive of anything so small that it cannot be divided, or so large that nothing can be added to it; but as the two opposite statements seem just as true, we find ourselves within two limits at each of which we confront something that must be so, but can't be so. Between these two irrationalities lie quantities we can handle rationally by mathematics.

[2] Thence "fore-and-aftness" and bilateral symmetry in organisms arose (doubtless modified by gravity). Bilateral symmetry seems the invariable result of the growth of what we may call, for lack of a better term, "untrammeled protoplasm." When protoplasm is "hampered," say by inorganic materials tending to produce other forms of symmetry—as, for instance, through the laws of crystallization—then bilaterality may be more or less masked; otherwise it is manifest. We readily recognize it in nearly all animals and plants.

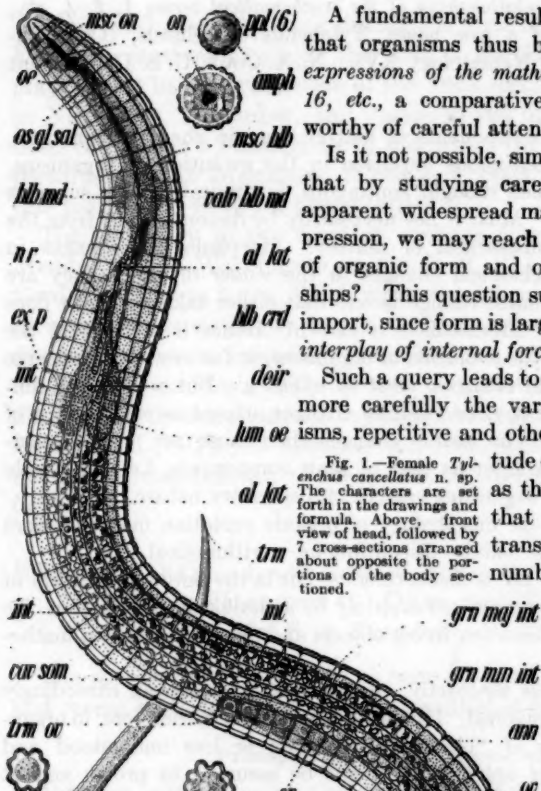


Fig. 1.—Female *Tylenchus cancellatus* n. sp. The characters are set forth in the drawings and formula. Above, front view of head, followed by 7 cross-sections arranged about opposite the portions of the body sectioned.

A fundamental result of this phenomenon is that organisms thus become *exact* or *modified expressions of the mathematical series 1, 2, 4, 8, 16, etc.*, a comparatively unheeded basic fact worthy of careful attention.

Is it not possible, simply by way of instance, that by studying carefully this more or less apparent widespread mathematico-biological expression, we may reach a clearer understanding of organic form and of phylogenetic relationships? This question suggests others of deeper import, since form is largely an expression of the *interplay of internal forces.*

Such a query leads to counting and comparing more carefully the various features of organisms, repetitive and otherwise. Among a multitude of others, such questions as this then arise: Why is it that both the number of transverse annules and the number of longitudinal elements in the cuticle of many nemas is likely to suggest some definite relationship to the geometrical series 1, 2, 4, 8, 16, etc.? The observed numbers are certainly cellular expressions of the 1, 2, 4 series, or variants, but why and according to what law is it that very often the numbers of elements met with are not members of the series but integers lying between? Why is it that chromosome-counts are suggestive of this same mathematical concept? And so on throughout the range of organic structures. Are not these

numbers not only necessarily and definitely, but perhaps somewhat simply, modified mathematical expressions of the fundamental mathematico-biological phenomena inevitably arising from the fact that cells (as well as some of their components) divide in accordance with the 1, 2, 4 series?

Variations of the 1, 2, 4 series, as expressed in cell multiplication, say in a segmenting egg, can be readily diagrammed. (See Fig. 3.) If in such a cell-division diagram any particular multiplying cell or cells be pictured as halted, while the others continue to divide, the next step will bring about a variation from the geometrical series. If the reader will draw a few simple diagrams, he will find it easy, by such variations, graphically to represent, as existing at successive early stages in the imagined ontogeny, numbers of cells, say, from 1 to 10 inclusive, and will see that conceivably this could go on indefinitely, and that therefore *any number whatever* is a possible biological variation of the 1, 2, 4 series. But this broadening of the possibilities must not be allowed to obscure the basic fact that the *numbers are nevertheless definite mathematical variations of the 1, 2, 4 series due to the binary division of cells and their components;*—which in turn seems compulsory owing to the nature of matter itself. Our problem seems to be: Which of these numerous variations are the more significant, and what are their *mathematical and biological relationships?*

A new triplonch, *Tylenchus cancellatus* n. sp. (Figs. 1 and 2), infesting the roots of peonies, will serve, in a very limited way, to illustrate the foregoing remarks. The figures (Fig. 1) show the existence, near the head, of sixteen external longitudinal grooves. Near the middle of the neck this number changes to eighteen by the splitting, on each side of the nema, of one of the lateral, or sublateral, elements of the series, so that most of the body presents 18 grooves. Posteriorly this number reduces to 14, 10, then 8. (Fig. 1.)

This emphasizes the value of pondering the *variants* of the 1, 2, 4 series. If the numbers of the various elements were confined to the 1, 2, 4 series, they would be less significant, hence less useful;—e.g., in the interpretation of relationships. But variations abound, and are, as yet, for the most part unexplained; probably often highly complex. It is certain, however, that *if these variations can be envisaged and understood,* they will serve as basic data.

Fig. 2. Peony root (*Paeonia officinalis*), nat. size; swellings (1, 2, and 3) contain *Tylenchus cancellatus.*

There seems at present no way of stating exactly, the upper limit of the numbers representing these variations of the 1, 2, 4 series as exemplified in an organism. It may in some organisms reach twenty figures, and therefore the discovery and interpretation of some of the highest members of this modified geometrical series, as exemplified in organisms, may be beyond our present compass. Nevertheless, does it not seem likely that relationships traced in this manner may at least be set upon a *firmer basis* than is the case when data of other sorts are used,—or even upon an *entirely new basis?*

In a 1, 2, 4 series, let P_N be the final product and N its series number, then $P_N = 2^{N-1}$; thus, $16 = 2^{5-1}$.

Similarly in a simple organism, at any particular instant in its growth, let P_N have a corresponding value,—that is to say, be the number of cells that either actually exist or *would have arisen by the uniform and continuous* dichotomous division of the single primal cell. Such simple and easily understood organisms occur among the lower forms, and in the early embryonic stages of the higher forms, but are rare among the adult stages of the higher forms, because in these latter some cells lag or cease in their dichotomy, and because of losses of cells from various causes. Hence, the number of cells actually present in an organism at any particular instant is likely to be *P_N minus a certain number of cells,* (X), due to delay or failure in some part or parts of the dichotomy, or to loss. In this discussion account is taken of *all* the cells that have been produced during the growth, whether present in the organism at the proposed instant or not. This is in order to allow for worn out or wasted cells; these, possibly vanished, cells are *included in P_N.*

The general 1, 2, 4 equation of an organism thus becomes $P_N = 2^{N-1} - X$,

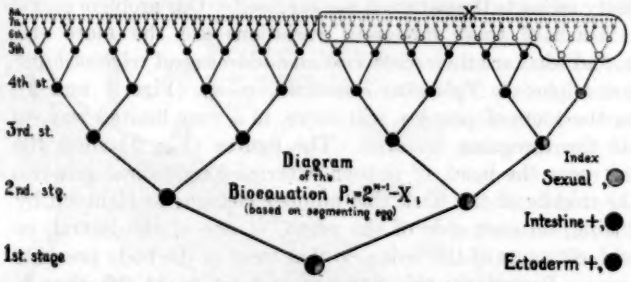


Fig. 3.—Diagram of 8 generations of cells produced by dichotomous divisions;—as, for instance, in a segmenting egg. Three general characters of tissue are shown: (1) Sexual, (2) intestinal and related tissues, (3) ectoderm and related tissues. The sexual and intestinal tissues are shown to have lagged behind those of the ectoderm, so that P_N in this instance equals 71.

in which *X is a whole number and a function of one or more "p's" of a lower* order, i.e., of the 1, 2, 4 character, or $p = 2^{n-1}$ character, in which, of course p is smaller than P and n is smaller than N. These smaller (ascertainable) groups are 1, 2, 4 groups of cells due to the lag or failure of *"earlier" generations than N.* (See the loop (X) in Fig. 3.)

$P_N = 2^{N-1} - X$ is a general equation, which, when $X = 0$, represents a strictly uniform and continuous mathematical dichotomy, found only in the lower organisms or in the early embryonic stages of the higher ones.

The various "p's" from P_N down to $P = 1$, (the primal number) become, therefore, *historical insignia, indicating particular generations of cells,* and may be made the basis of a *definite and fundamental mathematico-biological nomenclature* applicable to the generations of cells in an organism, and hence *to the organism itself.* Applications of the equation are endless.

GEOLOGY.—*On the correlation of certain Pleistocene deposits and their fossils.* OLIVER P. HAY, Carnegie Institution of Washington.

Recently, Dr. W. D. Matthew[1] presented a paper entitled "Correlation of the Tertiary formations of the Great Plains." As might have been expected, Dr. Matthew presents many interesting, instructive, and acceptable propositions; there are others from which one may be permitted to dissent. The tone of the paper is somewhat disquieting, and one gets the idea that the geology and paleontology of the Tertiary is in a bad way and needs the services of a reformer. Doubtless there is much justification for his pessimistic conclusions. However, it is not the Tertiary, in which he has done most of his work, that appears to Dr. Matthew so nearly in disorder, but the Pleistocene, both of North America and of Europe.

In two matters Dr. Matthew has recently afforded me gratification. In 1923[2] I ventured to refer the Blanco to the Upper Pliocene. Not long ago Dr. Matthew[3] made the same disposition of it. In a paper read before the Geological Society recently and published in this JOURNAL,[4] I recognized two Pleistocene faunas, an earlier and a later. In his first paper, Dr. Matthew states (p. 747) that it is adequately proven that there are differences between the earlier and the later faunas. However, considering the facility with which he moves up and down the dividing lines between formations, I fear Dr. Matthew will not long adhere to his decision concerning the Blanco proposition. As regards the earlier and later faunas, I doubt that his later and my later will coincide in either depth or superficies. I am further curious to learn where he finds the "proofs" of the differences alluded to.

Dr. Matthew thinks (p. 751) that it has not been settled whether the Equus beds of Nebraska and Kansas are at the base of the Pleistocene or on the top of the Pliocene. So far as I am aware, he is the only man who stands out, and he only of late. In his correlation table he places the Sheridan in the Pliocene and invites American geologists to accept the transference. It is a fair offer, and we shall await with interest their response. As an argument in favor of this new arrangement he states that the genus *Equus* is quite characteristic of the Upper Pliocene of Europe. This is a disputed point, one of those on which Dr. Matthew expresses an opinion in the fourth paragraph of his page

[1] Geol. Soc. Amer. Bull., **35**: 743–754. 1924.
[2] Carnegie Inst. Wash. Pub. No. 322, pp. 1, 15. 1923.
[3] Nat. Hist., **24**: 630. 1924.
[4] This Journal, **15**: 126–133. 1925.

752. Most European geologists have indeed drawn the line so as to throw a part of the Glacial period, the horses with it, into the Pliocene, but there is a reaction against this. In his great work[5] Émile Haug places the parting plane at the base of the Villafranchian, because then appeared, as he says, a new fauna, characterized by *Elephas*, *Equus*, and *Bos*, which pushed the old into the background. His arrangement throws into the Pleistocene the upper beds at Val d'Arno in Italy and the Norwich Crag in England. He further declares that the earliest glacial stage, the Scanian, immediately preceded the fluviomarine Cromerian, which itself presents the first interglacial fauna.

Recently, J. Reid Moir has published an article[6] on the human artifacts found in the vicinity of Cromer. Moir places the beds in the Pliocene, but regards the Cromer Forest bed as belonging to the first interglacial, and the Red and Norwich Crags as representing the first glacial epoch of East Anglia.

Moir's paper is immediately followed by one by Sir E. Ray Lankester, in which he expresses the opinion "that it is high time that the misapprehensions of Lyell and his followers should be discarded and the discoveries of the last 50 years given their true significance, by definitely assigning the Red and the Norwich Crags to the Pleistocene."

In the number of the Bulletin of the Geological Society which contains Dr. Matthew's paper is Dr. T. Wayland Vaughan's treatise entitled "Criteria and status of correlation and classification of Tertiary deposits," a communication replete with valuable information fortified with citations to geological literature. In his "Table No. 1," prepared under his direction by Dr. Julia Gardner, Vaughan presents the "Correlation of the Tertiary formations of Europe." He had to define the boundary between the Pliocene and the Pleistocene; and this he draws, as did Haug, below the Villafranchian in Italy and the Norwich Crag in England. It appears, therefore, that according to these authorities, the genus *Equus* is pretty well excluded from the Pliocene of Europe, and that Dr. Matthew has chosen an inauspicious moment to urge the incorporation of a part of the Glacial Epoch into our North American Pliocene. It is possible that species of *Equus* existed in Asia during the late Pliocene, but our classification of formations is not based on Asiatic geology. It is not impossible that species of *Equus* lived in Asia before any lived in America. Matthew tells us that *Hipparion* migrated to Asia and there developed species which were larger and more progressive than the native American forms. In

[5] Traite de Geologie, 1767. 1911.
[6] Nat. Hist., 24: 637–658. 1924.

like manner, another equine form may early have reached Asia and, under the stimulating environment of that continent, have given rise to advanced species of *Equus*. When the movement of the fauna of northern and temperate Asia into America began, those horses may have returned to the land of their ancestors. In the latest Pliocene known, the Blanco and the San Timoteo, close approaches to *Equus* are found. In the earliest Pleistocene that has furnished any considerable number of fossils there are numerous species of one-toed horses. It seems hardly possible that during that interval the genus *Pliohippus* should in America blossom out into more than a dozen species of *Equus*. Some of the early Pleistocene horses, as *Equus baudistensis*, *E. occidentalis*, and *E. idahoensis* are primitive in many respects. Others, as *E. fraternus*, *E. pectinatus*, *E. scotti*[7], all belonging to the early Pleistocene, are far more advanced. It appears possible that the primitive forms may have been natives of this country; the others have been Asiatic immigrants.

Dr. Matthew does not greatly appreciate the attempts made to correlate the Pleistocene faunas with the interglacial and glacial stages. He credits these correlations to Osborn and myself; but, inasmuch as Osborn has, to my knowledge, made no serious efforts in that direction, so far as America is concerned, I think that he at least might have been spared.

Dr. Matthew gives it as his opinion that the position and unity of the Aftonian are distinctly doubtful and its exact correlation with the Sheridan also questionable. He seems disposed to reach in and shake the insecure foundations of the correlations.

Iowa is the most interesting area of the North American glaciated regions, possessing sheets of drift of all the glacial stages and the deposits, erosions, and weatherings of all the interglacial times. Its glacial and interglacial phenomena have been investigated by probably a greater number of capable geologists than those of any other state, and their researches are yet being vigorously pursued. The glacialists welcome the suggestions made by all competent investigators and any contribution from Dr. Matthew will command attention.

A large part of the surface of the state is occupied by Kansan drift. This is known to extend, at some points at least, to the Missouri River and beyond. Beneath this drift sheet there exists, over much of the state, another, the oldest known, the Nebraskan. This too is known to reach the Missouri at points, and has been supposed to do so all

[7] See Iowa Geol. Sur., **23**: 183. 1914. Figs. 68, 69.

along the western border. Between these two drifts are found, over a large part of the state, evidences of a long interglacial stage when there was a mild climate and an abundant flora, doubtless an equally abundant fauna. During this time too the upper surface of the Nebraskan drift became deeply weathered and leached. This interglacial time is the Aftonian. Along the Missouri, near or not many miles away, where erosion has trenched the Kansan drift, or where it has been penetrated by wells and railway cuts, the bones and teeth of many mammals and the shells of land and freshwater mollusks have been discovered. These have been referred to the interglacial Aftonian.

Fossils have been discovered in supposed Aftonian deposits in about 20 localities. In 7 of these only mollusks were found; in 8, only mammal bones and teeth; in at least 5, both molluscan and mammalian remains. According to Shimek, 26 species of mollusks have been determined, and these all belong to species yet living in western Iowa. Eighteen at least of these species were found in one pit, at Turin. The unity, or homogeneity, of this molluscan assemblage can not be questioned. The species could not have been redeposited from older formations. Of the vertebrate species we may count about 25 species. Of these, 19 have been collected in one pit at Missouri Valley. At Turin, 8 species of vertebrates were collected, associated with the 18 or 20 species of the mollusks mentioned. Six of the vertebrates appear to be of the same species as those secured at Missouri Valley. The other two, *Megalonyx* and *Castoroides*, are common Pleistocene fossils.

The *Hipparion* remains found at Afton Junction, Iowa, may appear to be an intrusive relic of an older fauna. However, this is not the only case of the occurrence of *Hipparion* in the Pleistocene. Calvin reported a tooth of the genus from Rockport, Missouri, where it was associated with *Equus*, *Elephas columbi*, and a large camel, probably *Camelops*. The writer described a species of *Hipparion* collected by Cragin in western Kansas. In the same collection are *Mylodon*, two species of *Equus*, *Camelops*, *Elephas*, and *Canis*. That *Hipparion* was a companion of *Equus* in the early Pleistocene appears certain. Possibly some of those progressive species came to America with the Asiatic contingent early in the Ice Age. The facts presented show, I think, that the fauna found in the so-called Aftonian deposits of Iowa is a homogeneous one, not one made up of elements of two or more geological stages of epochs.

We may now ask whether these mammals and mollusks belong to the Aftonian or to another stage. I think all who understand the

question will agree that the fauna belongs somewhere between the beginning of the Nebraskan and the early part of the Kansan. Dr. George F. Kay, State Geologist of Iowa, has expressed the view that the remains are not found between the two drifts but in one or the other or both of them. I believe that this is an error which Dr. Kay will soon correct. The probabilities are strongly against the occurrence of sands and gravels containing bones and teeth and delicate shells within the drift in so many localities and along such an extent of country; especially since in all the rest of the glaciated region of North America bones or mollusks are rarely, if ever, found in the drift. Dr. Kay's theory is that the animals lived in front of a glacier re-advancing over ground which it had occupied not long before and then abandoned. Buried in the gravels of the earlier advance, the bones and teeth were covered over by the till of the return movement; and thus they are involved in the drift of one glacial stage and not between the drifts of two stages. Formerly, the apparent inclusion of some of those fossiliferous sands and gravels in the drift was explained on the theory that they had been ploughed up in frozen masses by the on-coming glacier.

Even if those mammals and mollusks lived near the approaching glacier, they were the not distant descendants of the species of the Aftonian and hence a part of the same fauna. However, the writer maintains that the animals found fossilized in those sands and gravels could not have lived anywhere near the glacial front. The plants and animals of British America could exist neither beneath the glacier nor upon it. They were obliged to move on south of it or perish. Wherever they were driven, the arctic animals, the musk-oxen, the reindeer, the birds, the fishes, had to have a climate to which they had been accustomed and the plants which they had learned to eat. The insects needed their arctic and subarctic climates and many a species required a particular kind of plant to which it had become structurely adapted. When the glacier was approaching Iowa, the state must have had a far colder climate than previously; and this climate did not form a narrow belt along the glacial front. Inasmuch as local glaciers existed in the mountains of Nevada and southern California, and even in the Andes near the equator, the average temperature of our whole continent was reduced. The seasons along the Gulf were cooler than before. From this chilled coast the average annual temperature decreased gradually northward to the arctic cold at the foot of the ice sheet. Perhaps one or two species of elephants, some musk-oxen and reindeer, and a few hardy carnivores, found existence

possible in Iowa; but we may be sure that the camels that had lived there, and the bisons, and the horses, and peccaries, and especially the ground-sloths, had long before abandoned the region, seeking better pastures.

Dr. Matthew states (p. 746) that the correlation of the Sheridan beds as the exact equivalent of the Aftonian is also questionable. One must not be too sure. Some three or four hundred miles out on the plains there might have existed a different species of muskrat or prairie dog, but the assemblages of mammals were probably as nearly alike as they are today. About 25 species have been found in the supposed Aftonian of Iowa and about 20 or more in Sheridan County, in each case doubtless only a small part of those existing in the localities. It is possible, too, that the Iowa animals lived a few hundred years earlier or later than those along Niobrara River. Hence, some of the differences may be explained. In the writer's opinion, there is not a species in either list which may not any day be added to the other.

Dr. Matthew ought to tell us where his Sheridan fauna belongs if not in the first interglacial stage. He must consider it an older fauna than the Aftonian, for he has chosen it to depress into the Upper Pliocene. If it does not belong to the first interglacial, corresponding to the Aftonian of Iowa and the Cromerian of England, he may assign it to the first glacial stage. Two objections present themselves. On the one hand, there is not a species which suggests a cold climate, no musk-ox, no reindeer. On the other, there are only *Megalonyx* and *Platygonus* that can be regarded as Pliocene species. Several other genera of remarkable mammals are known to have come high up into the Pliocene; but they do not appear in the Sheridan list. These Pliocene mammals must have been suddenly destroyed by the invading Asiatic hordes.

The reference of the Sheridan to the Pliocene does not quite satisfy Dr. Matthew. The Equus fauna must begin still further back. A new fauna containing *Equus*, found by Gidley in Arizona, is, Matthew tells us, considered as Middle or Upper Pliocene. It is apparently taken as older than the Sheridan and as falling outside the Glacial epoch. Apparently the Equus *entourage* has no known limits downward, just as in Dr. Matthew's correlation scheme it is not limited upward.

Now, to refer that Arizona fauna to the Pliocene is to prejudge the case. It is certainly younger than the Blanco and therefore nearer to the Sheridan. The larger animals, except a mastodon and a glypto-

don, have not been described. The mastodon is closely related to Sheridan and Aftonian species. The other mammals comprise 2 or 3 species of *Equus*, a *Pliohippus*, a *Hipparion*, a *Lama*, a *Procamelus*, a *Pliauchenia*, a peccary of possibly a known Pleistocene species, and deer of the genera *Odocoileus* and *Merycodus*. All of the rodents belong to existing genera and most of them are closely related to existing species. It is not necessary to step down directly from the Aftonian and the Sheridan into preglacial terranes. The possibility exists that the Arizona fauna belongs to the Nebraskan glacial stage. It has the transitional features to be looked for in such a position. It is possible, however, that Dr. Matthew does not recognize any Nebraskan stage; or if he does, he may think that it was a spell of weather rather disagreeable, but otherwise of little importance. In reality some of those barren "intermediate" beds (his p. 751) may find their place in the Nebraskan.

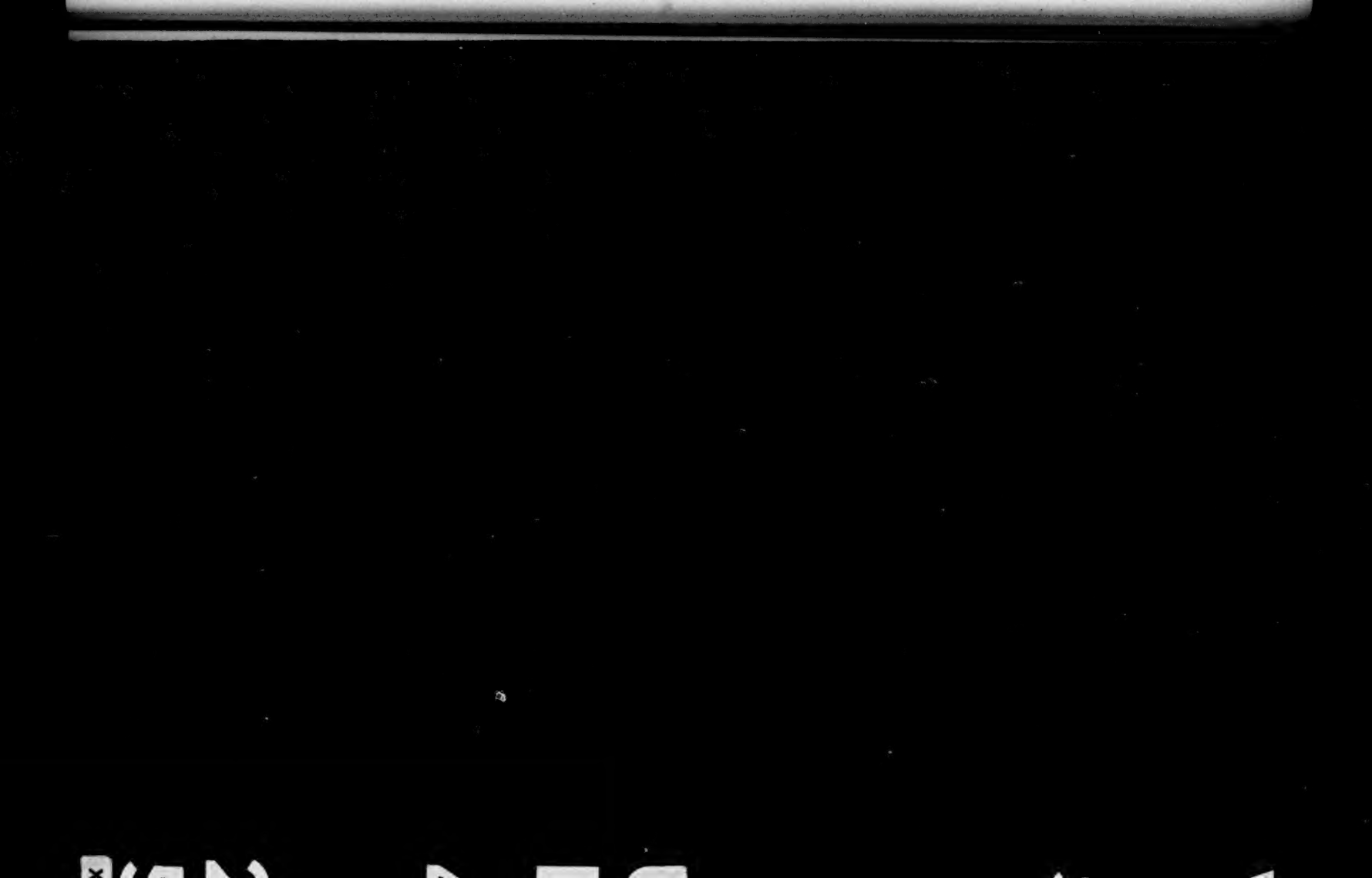
It is interesting to consider some other features of Dr. Matthew's correlation table. While he seeks to adjust the Sheridan nicely in the Pliocene he leaves the La Brea in the Pleistocene, and apparently not in the lowest. Yet most of the genera which he has named as characteristic of the *Equus* fauna are common to the two localities, *Canis*, *Arctotherium*, *Smilodon*, *Mylodon*, *Equus*, *Elephas*, *Camelops*, *Antilocapra*, *Bison*, and *Capromeryx*. We have here the representatives of an impressive assemblage of mammals which it is proposed, by the location of an arbitrary plane, to apportion to two distinct geological periods.

In the second column of Matthew's table no line is drawn between the Pliocene and the Pleistocene. Perhaps this treatment is as good as any, considering the prevailing uncertainty; and it might have been profitably applied in some other cases. We are told also that Osborn, Merriam, and Matthew have agreed to transfer the whole of the Hipparion zone to the Pliocene. I note with surprise that, as one of the contracting parties, my friend has not stood up to his bargain, having assigned the major portion to the Miocene. The impression one gets from that table is that the Tertiary faunas have very uncertain relations to the accepted geological periods; as is indicated also in the case of the Equus fauna. For example, it would now be hazardous to affirm that any species of *Hipparion* belonged to the Miocene.

Dr. Matthew has offered his suggestions regarding Pleistocene correlations in the hope that they will lead to fixity of views. They will scarcely do this. To refer the Sheridan beds to the Pliocene will be to exchange a natural and determinable plane of division for an arbi-

trary one, and will introduce into the Pleistocene the confusion that seems to have sway in the Tertiary. It will put us back to the time when one distinguished vertebrate palaeontologist referred our Pleistocene animals all to the Champlain stage, and another recognized two "faunas," which, however, during the whole Pleistocene, lived side by side and were really one and the same thing.

SCIENTIFIC NOTES AND NEWS

ALBERT H. KAMPE, who graduated in engineering from George Washington University in 1924, has been appointed observer on the staff of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington.

The Baltimore-Washington section of the American Ceramic Society met at the Lee House on Saturday, April 4. Program: H. G. WOLFRAM, of the Bureau of Standards: *Enamels;* J. W. GREIG, of the Geophysical Laboratory: *The formation of mullite from cyanite, andalusite, and sillimanite;* H. F. STALEY, of the Metal and Thermit Corporation: *Pottery and enameling practice on the Pacific Coast.*